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On the flaws in 'Official' HARP's data analysis

The HARP–CDP group

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CONTENTS

1 PROLOGUE

In 2003, after the end of data taking, a deep divergence of opinion erupted in the HARP Collaboration over issues of data analysis: strategies, priorities, quality criteria. This led to the separation of our HARP–CDP group from 'Official' HARP (OH) and to our independent analysis of HARP large-angle data. In the following, our results are labelled as CDP or HARP–CDP while the OH results are labelled as OH, HARP, or HARP Collaboration.

This paper[1)] presents:

1. the proof that OH's large-angle analysis is seriously flawed;
2. a reconstruction of the path toward wrong OH cross-sections; and
3. a critique of the 'track comparison' presented by the HARP–SPSC referees to the SPSC.

A complete account of our work can be found on our web page (http://cern.ch/harp-cdp).

2 OH'S LACK OF UNDERSTANDING TPC TRACK DISTORTIONS

2.1 Overview

The overriding problem in OH's data analysis is their lack of understanding TPC track distortions.

A few words to explain the issue.

The performance of the HARP TPC was affected by static and dynamic track distortions[2)]. The latter were primarily caused by the build-up of an Ar^+ ion cloud during the 400 ms long spill of the CERN PS. This ion cloud emanates from the TPC's sense wires and drifts toward the high-voltage membrane.

The dynamic track distortions increase approximately linearly with time in the spill. Their size in the $r \cdot \phi$ coordinate reaches at the end of the spill typically 15 mm at small radius. It exceeds the TPC's design $r \cdot \phi$ resolution of 500 μm by a factor of 30 and therefore requires very precise track distortion corrections.

In the HARP TPC, with a positive magnetic field polarity, dynamic distortions shift cluster positions such that positive tracks are biased toward higher $p_{\rm T}$ (conversely, negative tracks are biased toward smaller $p_{\rm T}$). OH chose—in principle correctly but not in the way they did it—to fit TPC tracks with the constraint of the beam point because the increased lever arm permits an approximate doubling of the $p_{\rm T}$ precision. While the beam point remains unaffected, the cluster positions get shifted by dynamic distortions. Assigning a sufficiently small position error to the beam point renders its weight (the inverse error squared) in the track fit so large that positive tracks now get biased toward lower $p_{\rm T}$, i.e., the trend of the bias is reversed with respect to the fit without beam point.

What we see in OH's published data, as explained in the following sections, is:

1) This revised version has expanded Sections 2.1 and 5.

2) The cause of these problems, the physics of the track distortions, their quantitative assessment, and their corrections, are described in Refs. [1]– [4].

- a bias of $\Delta(1/p_{\mathrm{T}}) \simeq 0.3$ (GeV/c)$^{-1}$ in their reconstruction of TPC tracks; in other words, their relative p_{T} bias increases linearly with p_{T} and attains some 30% at $p_{\mathrm{T}} = 1$ GeV/c; the bias is such that for particles with positive charge p_{T} is decreased, while for particles with negative charge p_{T} is increased;
- a resolution of $\sigma(1/p_{\mathrm{T}}) \simeq 0.6$ (GeV/c)$^{-1}$ which is considerably worse than $\sigma(1/p_{\mathrm{T}}) \simeq 0.30$ (GeV/c)$^{-1}$ claimed by OH; and
- a bad overall RPC time-of-flight resolution of 305 ps and an apparent advance of the timing signal of protons with respect to that of pions by ~ 500 ps ('500 ps effect').

These three problems, together with a number of additional mistakes that we list in Section 3, have the following fatal consequences for the determination of cross-sections of secondary hadron production:

- momentum spectra of secondary hadrons are distorted especially in regions where there is a strong momentum dependence; and
- protons and pions are partly confused and the abundances of positive pions are wrongly determined.

Our criticism of the OH analysis has been published in Refs. [5] and [6] in response to OH's insistence that their analysis is correct [7–13]. OH have published to date four physics papers with wrong large-angle cross-sections [14–17], in full conscience of our criticism of their work. To make things worse, the extensive TPC and RPC calibration work undertaken and published by our group [18, 19] is systematically ignored in OH publications and nowhere referenced.

2.2 The p_{T} bias

OH's argument that 'dynamic' TPC track distortions can be neglected during the first third of the 400 ms long accelerator spill, reads as *"...Owing to their limited mobility the first* [argon] *ions created in the amplification region need about 25 ms to reach the drift region and subsequently the steady flow of ions into this region only starts approximately 100 ms after the start of the spill, with a gradual transition between these two regimes..."* This argument is wrong. With an electric field strength of $\sim$1.7 kV/cm the argon ions need less than 1 ms for the relevant distance of 11 mm. Therefore, dynamic TPC track distortions increase right away approximately linearly with time in the spill. In order to appreciate the quantitative dimension of the problem: dynamic track distortions in the HARP TPC reach at the end of the spill $\sim$10 mm in the $r \cdot \phi$ coordinate, which is one order of magnitude larger than the typical $r \cdot \phi$ resolution. We have published the correct analysis of TPC track distortions in Ref. [18].

OH's claims *"...The constrained fit* [which uses the beam point in addition to the TPC clusters] *is unbiased with respect to the unconstrained fit* [which uses the TPC clusters only]..." and *"...The weight of the vertex constraint compensates perfectly the distortions..."* evidently contradict logic. It is impossible that a circle fit of TPC clusters that shifted away from their nominal $r \cdot \phi$ positions, together with the (undistorted) beam point, yields the same p_{T} as a fit of the distorted TPC clusters alone. It is equally impossible that a circle fit of the distorted TPC clusters together with the undistorted beam point gives an unbiased p_{T} estimate.

OH's justification of their claim of the absence of a bias in their circle fit stems from Fig. 4 in Ref. [12], reproduced here in the left panel of Fig. 1: *"...In Fig. 4 it is shown that the vertex* *constraint does not introduce biases for those particle trajectories and that the simulation provides an excellent description of the behaviour of the resolution function..."*. Their Fig. 4 claims to prove the equality of momentum from OH's 'constrained' and 'unconstrained fits'. That this claim is wrong is evident from the unphysical non-Gaussian shape of the shown distribution. Its cause is a mistake in their calculation of the $r\cdot\phi$ error of TPC clusters: their $\sigma^2_{r\phi}$ is multiplied by a factor $\cos^2 2\phi$ which assigns clusters an unphysically large weight depending on how close they are to the singular values $\phi = 45°$, 135°, 225° and 315° in the azimuthal angle. The mathematical intricacies of this mistake are explained in Ref. [20]. Our simulation [5] of the consequences of this mistake, shown in Fig. 1, is telling.

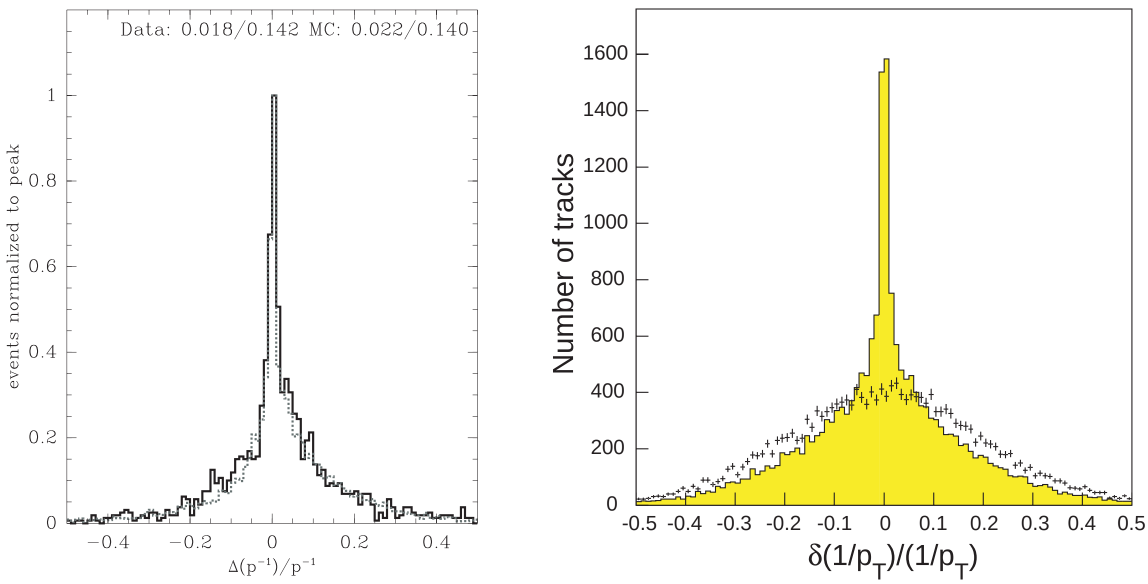

Fig. 1: Left: OH's comparison $(p1 - p2)/p2$ of the fit without beam point ($p1$) and with the beam point ($p2$), for data (full line) and Monte Carlo (dotted line); this figure is a copy of Fig. 4 in Ref. [12]; right: our simulation of the expected Gaussian distribution of $(p1 - p2)/p2$ (crosses) and of its deformation (shaded histogram) when OH's wrong $\cos^2 2\phi$ factor (see text) is applied to the $\sigma^2_{r\phi}$ of TPC clusters.

OH never presented evidence that after TPC track distortion corrections their $r\cdot\phi$ residuals with respect to an external coordinated system are compatible with zero across the whole active TPC volume.

In their most recent physics publication [17], OH claim *"...Corrections that allow the use of the full statistics to be made, correcting for such* [dynamic] *distortions, have been developed...and are fully applied in this analysis. The obtained results are fully compatible with the statistical errors and differential systematic uncertainties with those previously published..."*. This claimed agreement between data from the first third of the spill without distortion correction, with data from the full spill with distortion correction, permits the conclusion that OH's full-spill analysis is beset by the same flaws as their earlier analysis of data from the first third of the spill. This is of particular relevance in conjunction with what OH show in their Fig. 8 in Ref. [12], reproduced here as Fig. 2: the *"Average momentum of particles with a* $\mathrm{d}E/\mathrm{d}x$ *in the TPC corresponding to 7–8 MIP, as measured in 31 different settings. The horizontal dashed lines correspond to a* *variation of ±2% around the average value of 340 MeV/c. The different settings are labeled with the material of the target and the momentum, in GeV/c, of the incident beam"*. OH should have realized that the scatter of the points in the said figure is not statistical. Rather, it reflects the strength of dynamic TPC track distortions in the various settings. In this plot that purports to demonstrate that OH's TPC momentum scale is correct, one reads off the contrary: that their p_T bias is an omnipresent phenomenon in all their large-angle analyses.

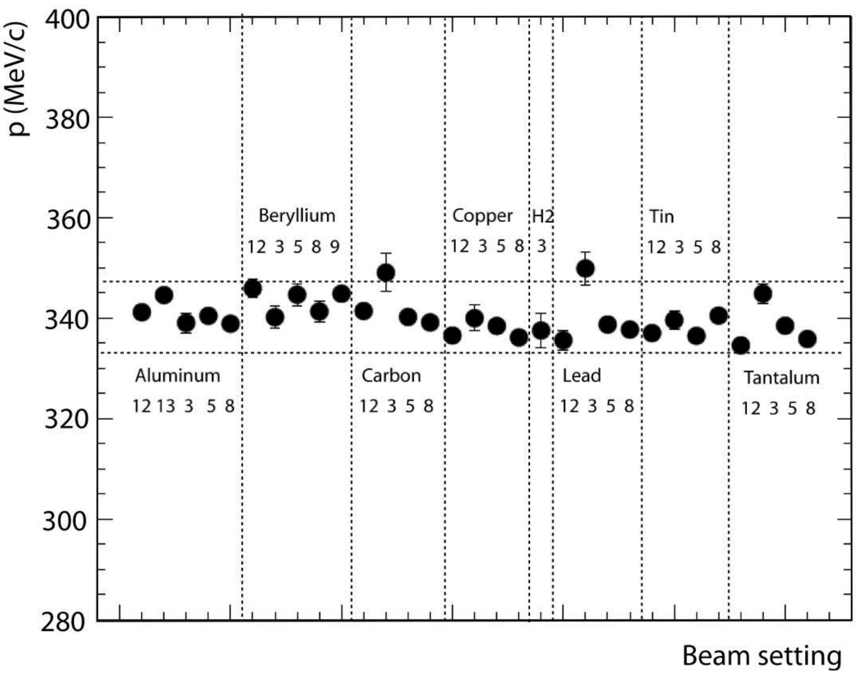

Fig. 2: Demonstration that the p_T bias in the OH large-angle data analyses is an omnipresent phenomenon; the figure is a copy of Fig. 8 in Ref. [12].

2.3 The bad p_T resolution

OH never provided evidence that their p_T resolution during the accelerator spill is indeed $\sigma(1/p_\mathrm{T}) \simeq 0.30$ (GeV/c)$^{-1}$ as claimed by them. Rather, they published the p_T resolution as measured by cosmic muon tracks—outside the accelerator spill and therefore not affected by dynamic TPC track distortions. We quote from Ref. [17]: *'The momentum and angular resolution were determined by exploiting the two halves of cosmic rays crossing the TPC volume'*. The reader is misled to believe that the same resolution holds also during the accelerator spill.

There is quite some published evidence for a very bad p_T resolution in the OH analysis. These are the width of the momentum distribution for a slice of $\mathrm{d}E/\mathrm{d}x$, the dip for large momentum in the q/p_T spectrum, and the effect of OH's '45° bug'. In addition, their overall RPC timing resolution of 305 ps proves that their p_T resolution cannot possibly be as good as $\sigma(1/p_\mathrm{T}) \simeq 0.30$ (GeV/c)$^{-1}$. Furthermore, their momentum distribution of recoil protons from the elastic scattering of beam protons on hydrogen is consistent with $\sigma(1/p_\mathrm{T}) \simeq 0.60$ (GeV/c)$^{-1}$.

2.4 The '500 ps effect'

Since OH have a biased track momentum, they observe that the RPC timing signal of protons is advanced by ~ 500 ps with respect to the RPC timing signal of (relativistic) pions. This observation led them to conclude *"...The detector physics of the RPCs is not well enough understood to use them as a calibration device at the few 100 ps level..."*. As a consequence, they made no use of the powerful particle identification capability from RPC time of flight. The exclusive use of $\mathrm{d}E/\mathrm{d}x$ from the TPC in conjunction with a biased track momentum is a major ingredient to wrong OH cross-sections.

OH's interpretation of their '500 ps effect' is characterized by statements like *"...For heavily ionizing particles the statistics is much larger so that stochastically, the first avalanche starts earlier..."* or *"...An order of magnitude estimate of the effect given the propagation velocity of electrons in the gas and the chamber gap leads to an order of magnitude of a few 100 ps..."*. This understanding of signal generation is wrong. The anode signal is generated by induction. Hence the (fast) propagation of electromagnetic waves across the gas gap is relevant and not the arrival at the anode of—in comparison—slowly moving electrons. (By contrast, we have used the established understanding of the mechanism of RPC-signal generation in our analysis [19].)

Figure 3 shows that OH's time advance of protons (black points; data from Ref. [11]) is satisfactorily explained by our simulation of the time advance that results from a bias $\Delta(1/p_{\mathrm{T}}) \simeq 0.30$ (GeV/c)$^{-1}$. There is no need and no room for OH's claim of a novel detector physics effect in timing RPCs.

2.5 Inadequate pion–proton separation

Because of their wrong RPC calibration, OH cannot use time of flight for particle identification. We quote their tell-tale argumentation: *'The choice to use $\mathrm{d}E/\mathrm{d}x$ as principal PID estimator is motivated by two facts. The first argument is given by the fact that $\mathrm{d}E/\mathrm{d}x$ is obtained as a property of the same points which constitute the TPC track, while the TOF is obtained by matching the track to an external device. It is observed that the background in the matching is not negligible. Converted photons from π^0 production can hit the same—rather large—RPC pad as the one pointed to by the track. This background depends on the position in the RPC barrel where the pad is located and is different for every momentum setting. Thus a different background subtraction would have to be determined for each momentum–target dataset. The second argument is the increased complexity of the analysis which would be introduced by having to combine two PID detectors of which the response is highly non-Gaussian. The probability density functions of both the response of the $\mathrm{d}E/\mathrm{d}x$ and of the TOF would have to be determined as function of all relevant parameters. The gain in efficiency one would obtain with such a procedure would be rather limited and would not balance the additional systematics introduced.'*

Figure 4 demonstrates how off-scale OH's assessment of the situation is. Figure 4 (a) shows our own measurement of the specific ionization $\mathrm{d}E/\mathrm{d}x$ in the TPC, and Fig. 4 (b) our measurement of the relative velocity β from the RPC time of flight, of positive and negative secondaries, as a function of the momentum measured in the TPC. The figures demonstrate that in general protons and pions are well separated. They also underline the crucial importance of the RPC time of flight for the separation of protons and pions at large momentum. This is a decisive advantage in comparison with OH's analysis who discarded the RPCs and used only $\mathrm{d}E/\mathrm{d}x$ for

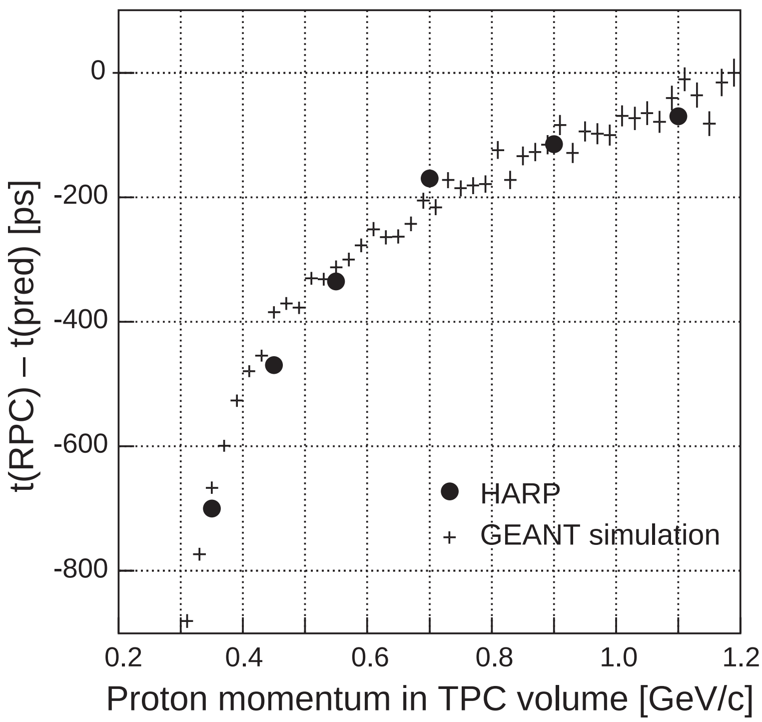

Fig. 3: OH's time advance of protons (black points; data from Ref. [11]) compared with our simulation of the time advance that will result from a bias $\Delta(1/p_{\mathrm{T}}) \simeq 0.30$ (GeV/c)$^{-1}$.

proton–pion separation. This has two consequences: first, their analysis is limited to particle momenta below $\sim$0.8 GeV/c; second, they confuse pions and protons and determine wrong positive pion abundances.

3 Further flaws in the OH data analysis

Further to the above list of major flaws in the OH analysis, we merely list the following additional flaws (a detailed discussion can be found in Refs. [20–22]):

1. OH's TPC cluster reconstruction is biased; their z position of clusters is calculated from the leading edge of clusters.
2. OH's normalization of TPC pads with 'superevents' is biased; it depends on beam momentum and target type.
3. OH determined a biased electron drift velocity; in the region between target and endcap of the inner TPC field cage the dependence on the strong electric field distortions was ignored.
4. As for static TPC distortions: OH ignore the 'Durchgriff' and 'Margaritka' distortions.
5. OH's determination of the track reconstruction efficiency by comparing with a Monte Carlo simulation is not adequate.

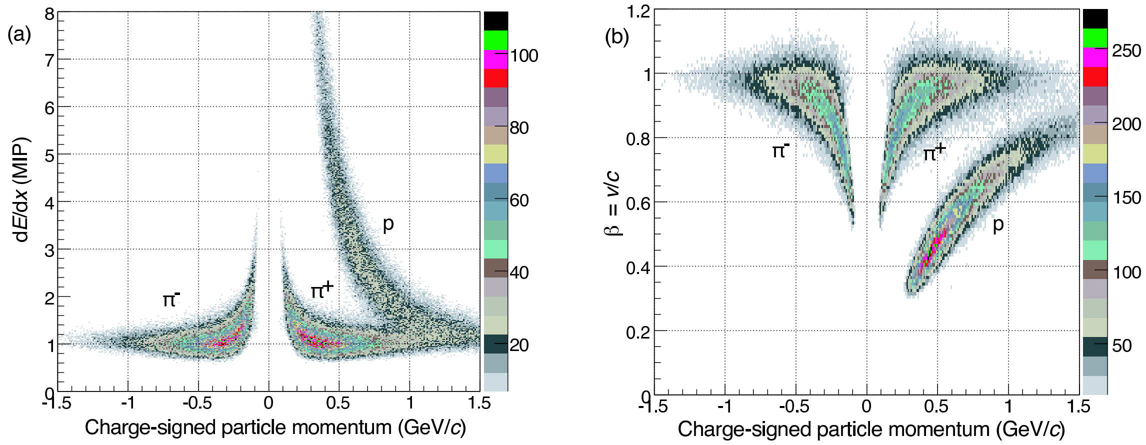

Fig. 4: Our measurements of the specific ionization $\mathrm{d}E/\mathrm{d}x$ (a) and of the velocity β (b), versus the charge-signed momentum of positive and negative tracks in $+8.9$ GeV/c data.

6. OH's handling of the energy loss in materials between the vertex and the active TPC volume, in fits that involve the vertex point, is wrong. Apart from generally biasing cross-sections, this prevents them from giving cross-sections for proton production, for the proton energy loss is much larger than the energy loss of (relativistic) pions.
7. OH never specified which Geant4 physics list they used for their analysis. This is important because on the one hand OH rely heavily on corrections from Monte Carlo simulation, on the other hand it is known that serious flaws exist in Geant4 hadronic physics lists as we showed in Ref. [23]. We know that the use of the existing Geant4 hadron generators is not possible. There is no single word on this in OH's papers which raises the spectre that they did not even realize the problems.
8. In OH's geometry, the barrel RPCs are incorrectly rotated by 1° in the azimuthal angle, apparently in an attempt to 'correct' for TPC track distortions.
9. Since OH do not use the RPCs, they rely on the subtraction of an important fraction of the electrons by Monte Carlo; we have shown that the RPCs are perfectly able to identify electrons experimentally, they had indeed been designed and constructed for this purpose.
10. OH claim to correct simultaneously for B-field inhomogeneity, experimental resolution, energy-loss in materials, trigger efficiency, track reconstruction efficiency, acceptance, backgrounds, absorption, decays, and tertiary interactions, in a single 'unfolding prodecure'. Knowing the problems behind all this from own experience, and knowing of shortcomings in the OH Monte Carlo simulation, we are not convinced at all of this approach.
11. OH published cross-sections that are not corrected for a trivial effect: *'We do not make a correction for the attenuation of the proton beam in the target, so that strictly speaking the cross-sections are valid for a* λ_{I} *= 5% target.'*

In view of the above, we hold that calibrations and cross-sections published by OH cannot be trusted. This refers explicitly to their four physics papers [14–17], four technical papers [7, 9, 11, 12], two Rebuttals [8, 10], and one Comment [13], published to date.

4 COMPARISON OF CDP AND OH CROSS-SECTIONS

Figure 5 a shows the comparison of our cross-sections of pion production by $+12.0$ GeV/*c* protons off beryllium nuclei with the ones published by OH [17], in the polar-angle range $0.35 < \theta < 0.55$ rad. The OH cross-sections are plotted as published, while we expressed our cross-sections in the unit used by them. Figure 5 b shows our ratio π^+/π^- as a function of the polar angle θ in comparison with the ratios published by the E910 Collaboration (at the slightly different proton beam momentum of $+12.3$ GeV/*c*) and by OH.

The discrepancy of the results published by OH with our results, but also with the results from E910, is evident. The discrepancy with our results is the more striking as the same raw data were analysed by OH and by us.

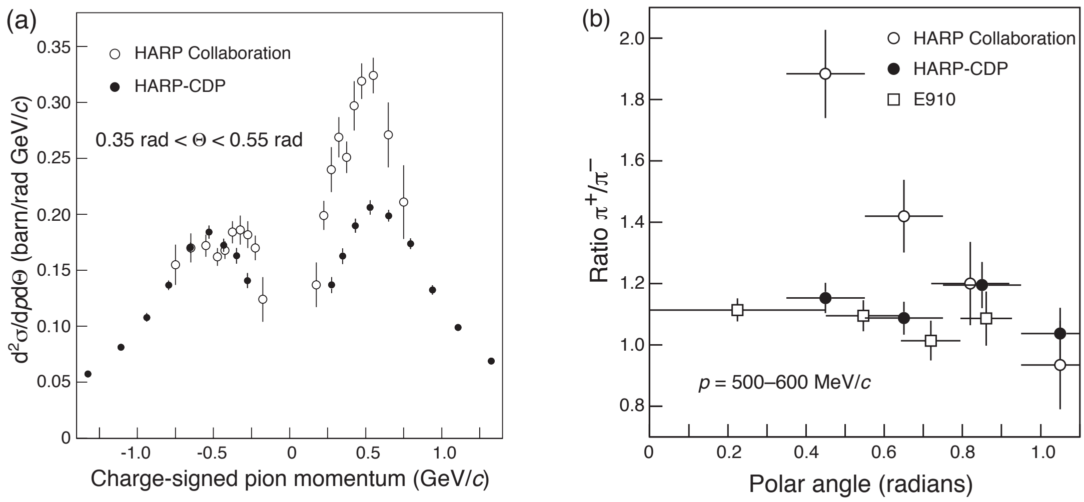

Fig. 5: (a) Comparison of our cross-sections (black circles) of $\pi^{\pm}$ production by $+12.0$ GeV/*c* protons off beryllium nuclei with the cross-sections published by the HARP Collaboration (open circles); (b) Comparison of our ratio π^+/π^- at $+12.0$ GeV/*c* beam momentum as a function of the polar angle θ with the ratios published by the HARP Collaboration; also shown are the ratios π^+/π^- published by the E910 Collaboration for a $+12.3$ GeV/*c* beam momentum; for the HARP Collaboration, total errors are shown; for E910 and our group, the shown errors are statistical only.

We note that the features that distinguish the cross-sections published by OH from ours are consistent with what we concluded from our assessment of their analysis procedures. For a quantitative analysis, we refer to Section 5.

5 A RECONSTRUCTION OF OH'S PATH TOWARD WRONG CROSS-SECTIONS

Here, we attempt to reproduce semi-quantitatively OH's path toward wrong cross-sections[3)].

[3)] This Section is added in response to strong pressure from the SPSC's HARP referees to elucidate the discrepancy between CDP and OH cross-sections.

We take our tracks from the interactions of $+8.9$ GeV/c protons in a 5% $\lambda_{\rm abs}$ beryllium target. Our tracks are correctly measured and identified and referred to as 'CDP-tracks'. To reproduce the OH analysis, we impose artificially on our tracks a bias of $\Delta(1/p_{\rm T}) \simeq 0.3$ (GeV/c)$^{-1}$. The biased tracks are referred to as 'OH tracks'.

Figure 6 shows the effect of this bias. We stress that the bias of 0.3 (GeV/c)$^{-1}$ is to be only understood as 'typical'. If asked to quote an uncertainty on this number, from our experience we would quote a range from 0.25 to 0.5 (GeV/c)$^{-1}$. Dynamic TPC track distortions depend on beam energy, beam polarity, beam intensity, beam scraping, target type, photon conversion in materials, and spiralling low-momentum electrons. They have a complicated dependence on radius, the z coordinate, and the time in the spill. There are several parameters which have to be individually determined for each beam energy, beam polarity, and target ('setting'), their effect can only approximately be condensed into a single number[4)].

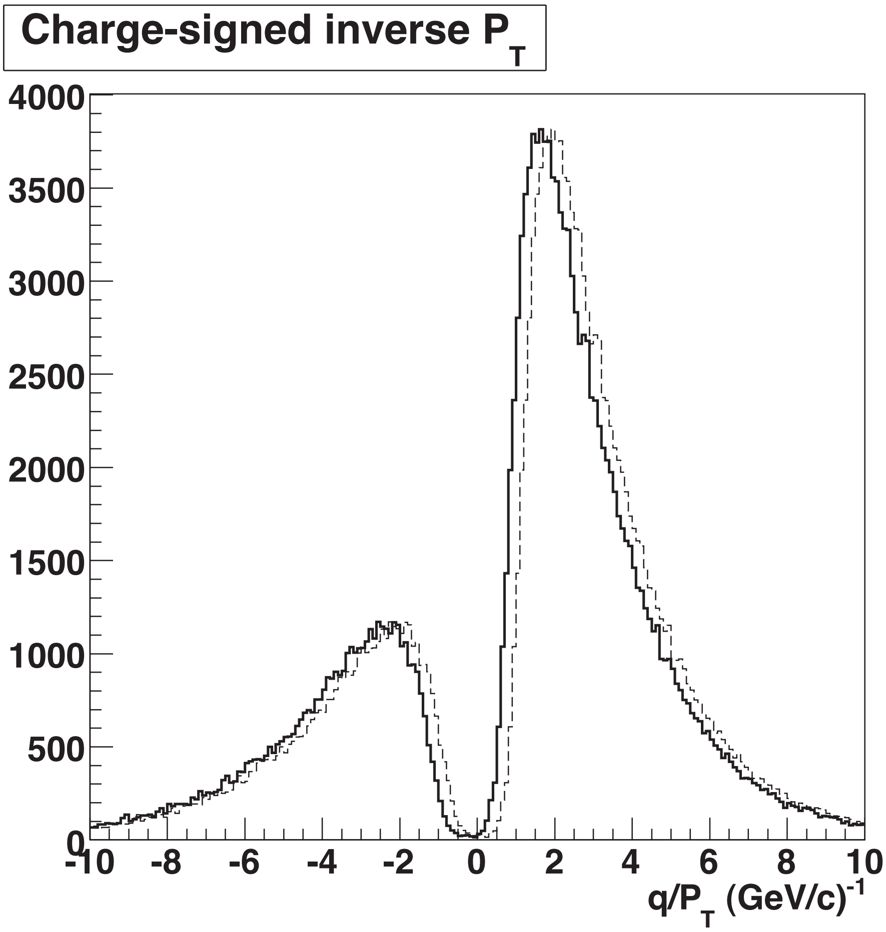

Fig. 6: CDP data: all secondaries from $+8.9$ GeV/c proton interactions in a 5% $\lambda_{\rm abs}$ beryllium target versus charge-signed inverse $p_{\rm T}$; the full line is the spectrum with unbiased momentum, the broken line shows the same tracks with a bias of $\Delta(1/p_{\rm T}) \simeq 0.3$ (GeV/c)$^{-1}$ imposed.

[4)]For reasons of the complexity of dynamic TPC track distortions, we do not have a single 'typical' number for each setting; therefore, repeating the analysis presented in this Section for other settings makes no sense, for the use of the 'typical' bias of 0.3 (GeV/c)$^{-1}$ is to show the generic features of the OH analysis and not the ones of a specific setting.

Figure 7 shows the $\mathrm{d}E/\mathrm{d}x$ distributions of CDP tracks for positive and negative secondaries. As for particle identification: while CDP assign a probability to every track to be a proton, a pion, or an electron, OH employ an analytical cut in the plane of $\mathrm{d}E/\mathrm{d}x$ versus momentum which selects pion candidates. Figure 7 shows this analytical cut, taken from Fig. 20 in Ref. [14], for positive and negative CDP tracks, respectively. The cut is severe and emphasizes purity over efficiency.

The efficiency of OH's pion selection cut when applied to negative pions that were identified with the CDP particle identification algorithm, is shown as histogram in Fig. 8. It is somewhat different from the black triangles which give the pion selection efficiency published by OH in the left panel of Fig. 21 in Ref. [14]—we will return to this point later[5)].

The momentum bias in the OH analysis is corroborated by the upper panels in Figs. 9 and 10 that are copied from the right panel of Fig. 18 and the left panel of Fig. 19 in Ref. [14], in comparison with the respective lower panels that show CDP data, and CDP data with an artificial momentum bias of $\Delta(1/p_{\mathrm{T}}) \simeq 0.3$ (GeV/c)$^{-1}$. Fig. 9 shows the $\mathrm{d}E/\mathrm{d}x$ distribution for momenta between 300 and 350 MeV/c, Fig. 10 the same for momenta between 500 and 600 MeV/c. The horizontal scales of the CDP and OH data sets are made comparable by equating the origin and the pion peak. The $\mathrm{d}E/\mathrm{d}x$ ranges are the same although the units are different. It is evident that the broad proton peaks are incompatible with each other. Because of the bias in the momentum measurement, the OH proton peak shows the specific ionization for momenta which are in reality higher, and therefore the specific ionization is shifted toward smaller values[6)]. The shift is broadly consistent[7)] with a momentum bias $\Delta(1/p_{\mathrm{T}}) \simeq 0.3$ (GeV/c)$^{-1}$.

Next, we make 'OH tracks' out of 'CDP tracks' that are identified as pions in the CDP analysis, by applying a bias of $\Delta(1/p_{\mathrm{T}}) \simeq 0.3$ (GeV/c)$^{-1}$. The resulting spectral distortion is striking and shown in the upper panels of Figs. 11 and 12. We show two ranges of polar angle, $20^\circ < \theta < 30^\circ$ and $70^\circ < \theta < 90^\circ$.

For $70^\circ < \theta < 90^\circ$ the discrepancy between the CDP and OH spectra is less striking but still visible.

Then, we reproduce OH's algorithm to obtain the number of pions. We apply OH's pion selection cut and correct the number of positive pion candidates for the cut's efficiency. For this, we take the efficiency published by OH and shown as black triangles in Fig. 8. As shown in the lower panels of Figs. 11 and 12, the enhancement of the OH positive pion spectrum over the CDP spectrum becomes more pronounced while the disagreement in the negative pion spectrum is reduced.

For negative pions there is no reason, though, to apply the pion selection cut and then correct for it. There are no antiprotons that contaminate the sample of negative pions. Therefore, errors in the pion selection efficiency should have no effect on the negative pion spectrum.

We have not attempted to reproduce the contamination of the OH sample of positive pions by

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[5)] An uncertainty at the few per cent level between the pion m.i.p. from CDP and OH was ignored; the pion m.i.p. is not too well determined because of electronic cross-talk in the TPC pads which primarily affects small pulseheights; the correction for electronic crosstalk is different between CDP and OH.

[6)] Here, beryllium target data from CDP are compared with tantalum target data from OH which explains the difference in the abundances of pions and protons.

[7)] In this specific example, the data require an even larger bias.

protons. We note that at large momentum there are many more protons than pions, therefore even a small contamination of the pion sample by protons causes a large effect. The physics is such that protons are concentrated at small rather than large polar angle. As for the level of contamination in the OH analysis: this would necessitate the quantitative understanding of the OH resolution tails of protons in terms of momentum and $\mathrm{d}E/\mathrm{d}x$. We point to the tell-tale difference in the separation of pions from protons in the OH and CDP analyses, clearly visible in Figs. 9 and 10. We remind in this context that in the OH analysis the bad TPC sectors 2 and 5 are used which we chose to ignore for their bad quality. We consider the undertaking to understand the tails of OH's $\mathrm{d}E/\mathrm{d}x$ distributions equally hopeless and useless, in much the same way as trying to understand a mismatch of tracks between CDP and OH at the $\sim$10% level.

Summarizing, OH cross-sections are mainly, but not exclusively, wrong because of

- spectral distortions owing to OH's p_{T} bias;
- errors in the OH pion selection efficiency;
- contamination by protons.

We stress that this lists only the gross errors in the OH analysis. There are quite a number of additional effects (see also Section 3) which contribute to getting the OH cross-sections wrong.

We consider the striking cross-section differences between CDP and OH understood.

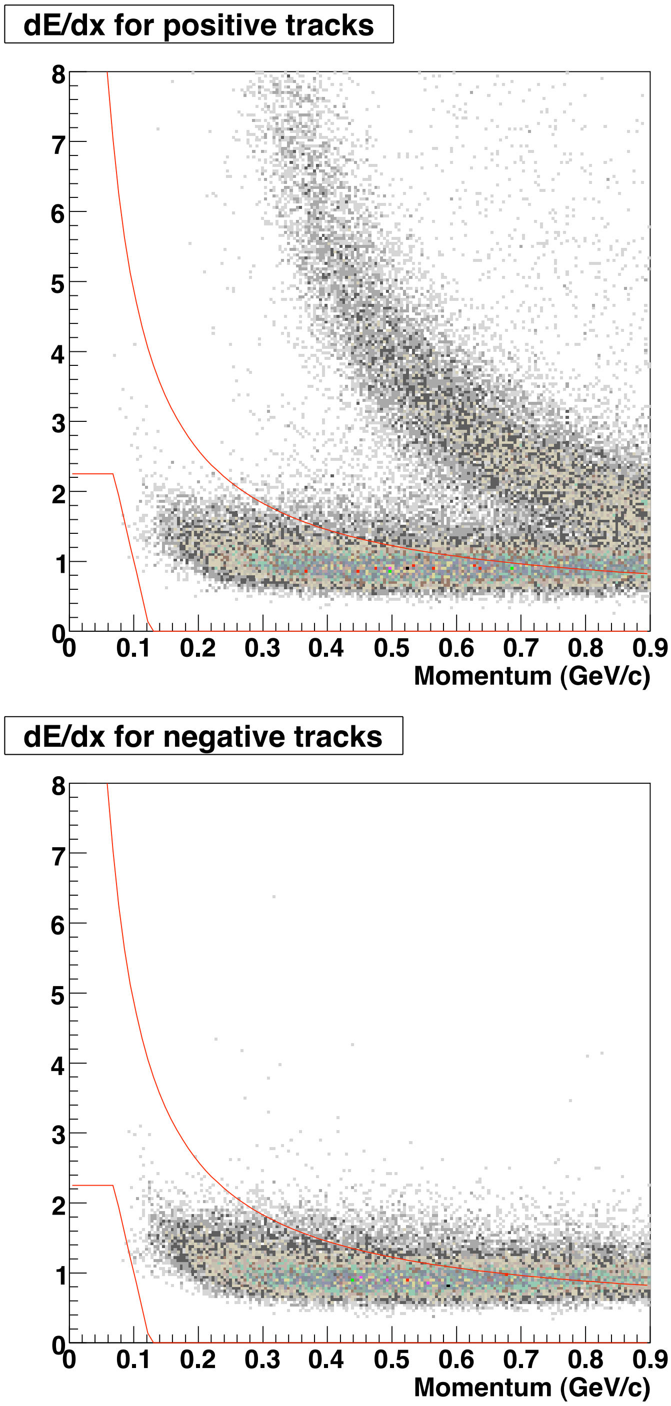

Fig. 7: $\mathrm{d}E/\mathrm{d}x$ distributions of CDP tracks together with OH's analytical cut to select pion candidates, for positive (upper panel) and for negative tracks (lower panel); the line represents OH's pion selection cut, the area at low $\mathrm{d}E/\mathrm{d}x$ and low momentum represents OH's electron selection.

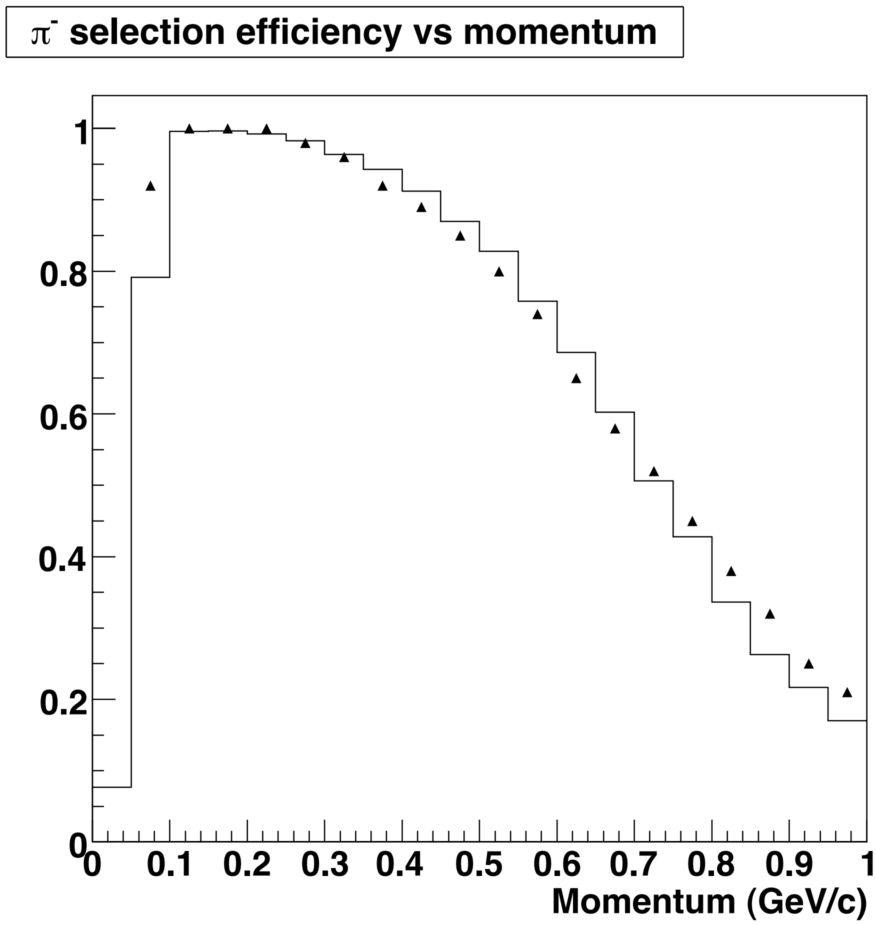

Fig. 8: The OH pion selection efficiency as determined from CDP-identified negative pions (histogram), and as published by OH (black triangles).

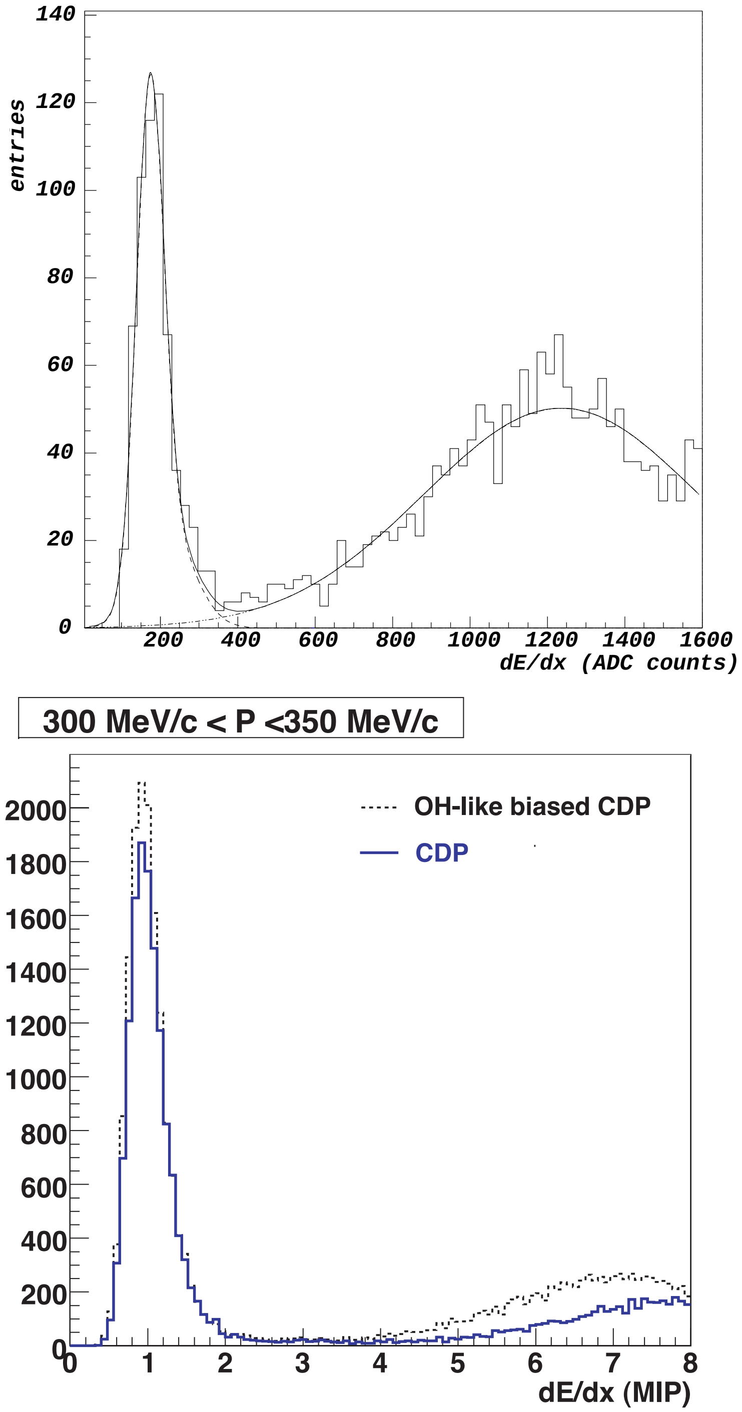

Fig. 9: $\mathrm{d}E/\mathrm{d}x$ distribution of positive tracks for momenta between 300 and 350 MeV/c; the upper panel shows OH secondaries from a Ta target and is copied from Ref [14], the lower panel shows CDP secondaries from a Be target tracks without and with an OH bias.

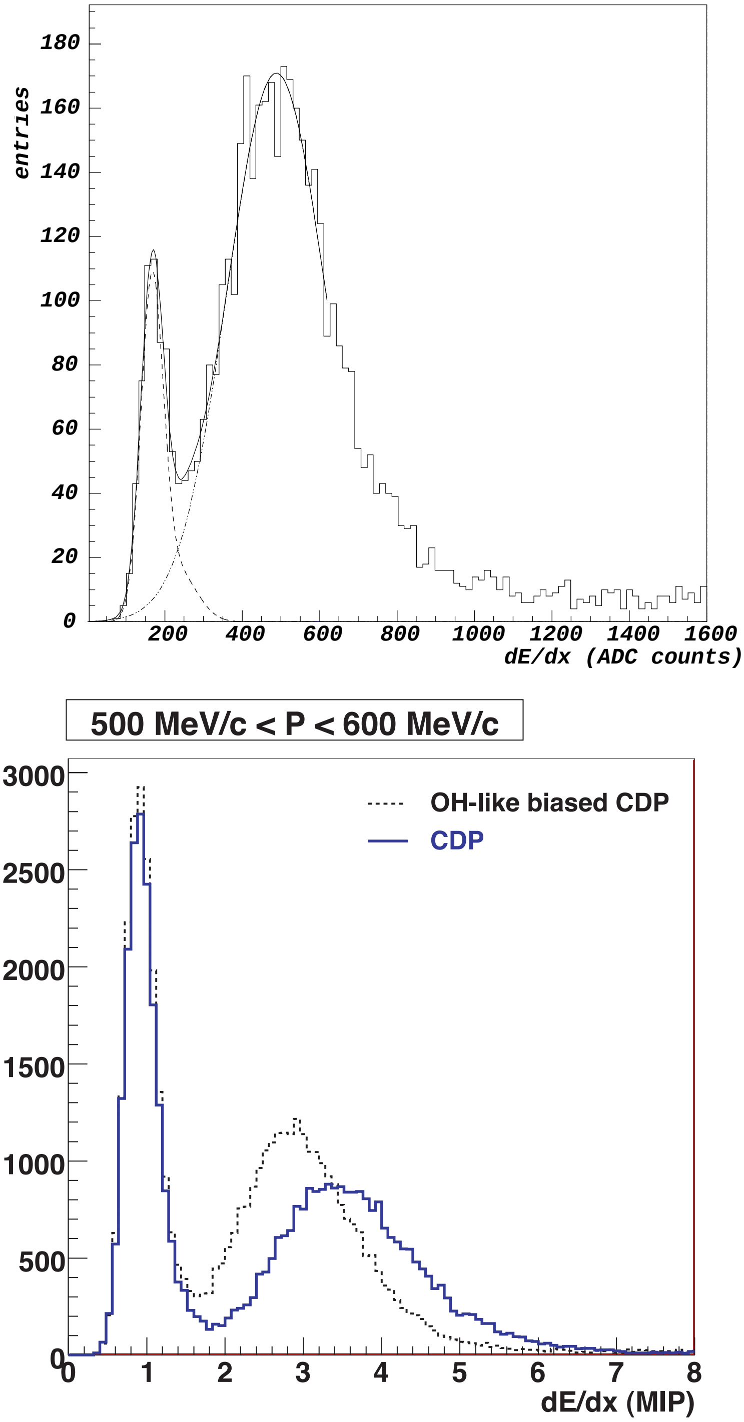

Fig. 10: $\mathrm{d}E/\mathrm{d}x$ distribution of positive tracks for momenta between 500 and 600 MeV/c; the upper panel shows OH secondaries from a Ta target and is copied from Ref [14], the lower panel shows CDP secondaries from a Be target tracks without and with an OH bias.

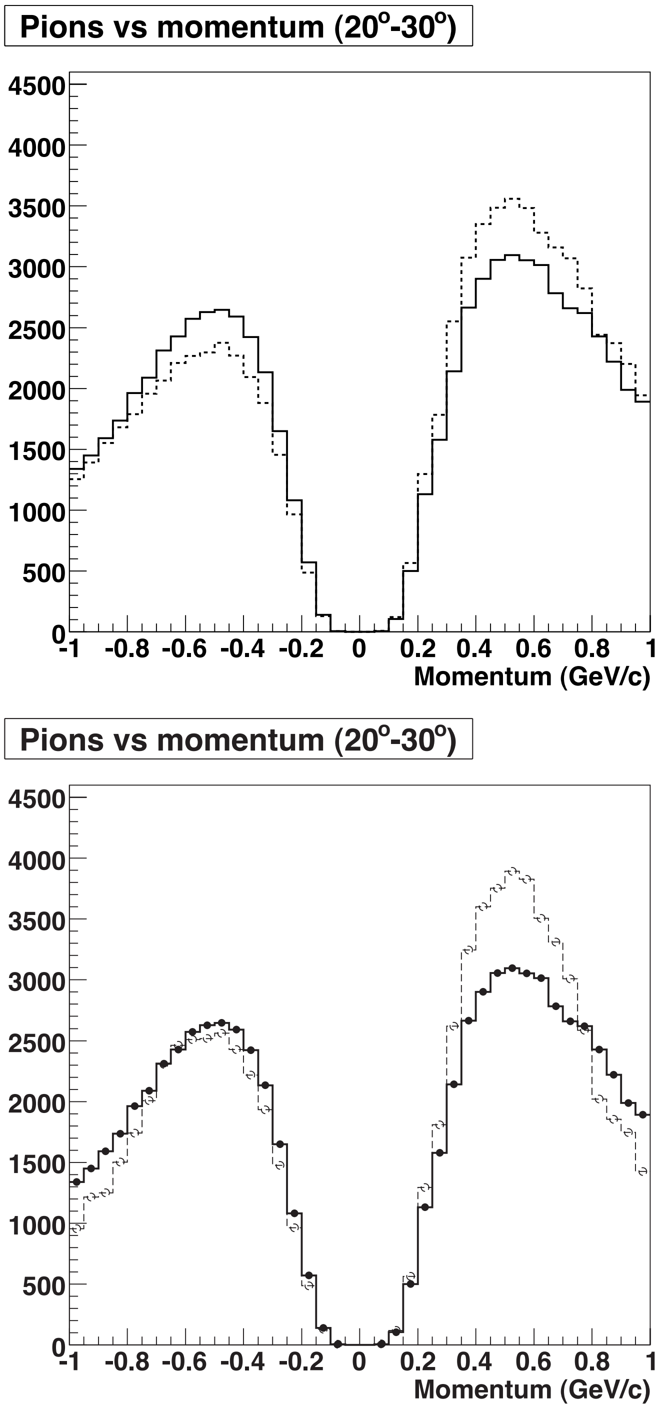

Fig. 11: Number of pions in the polar-angle range $20^\circ < \theta < 30^\circ$ from the CDP analysis (full line) and from the analysis that reproduces the OH analysis (broken line), as a function of the charge-signed pion momentum; the upper panel shows the spectral distortions caused by the OH momentum bias, the lower panel has the effect added from the use of the published OH pion slection efficiency.

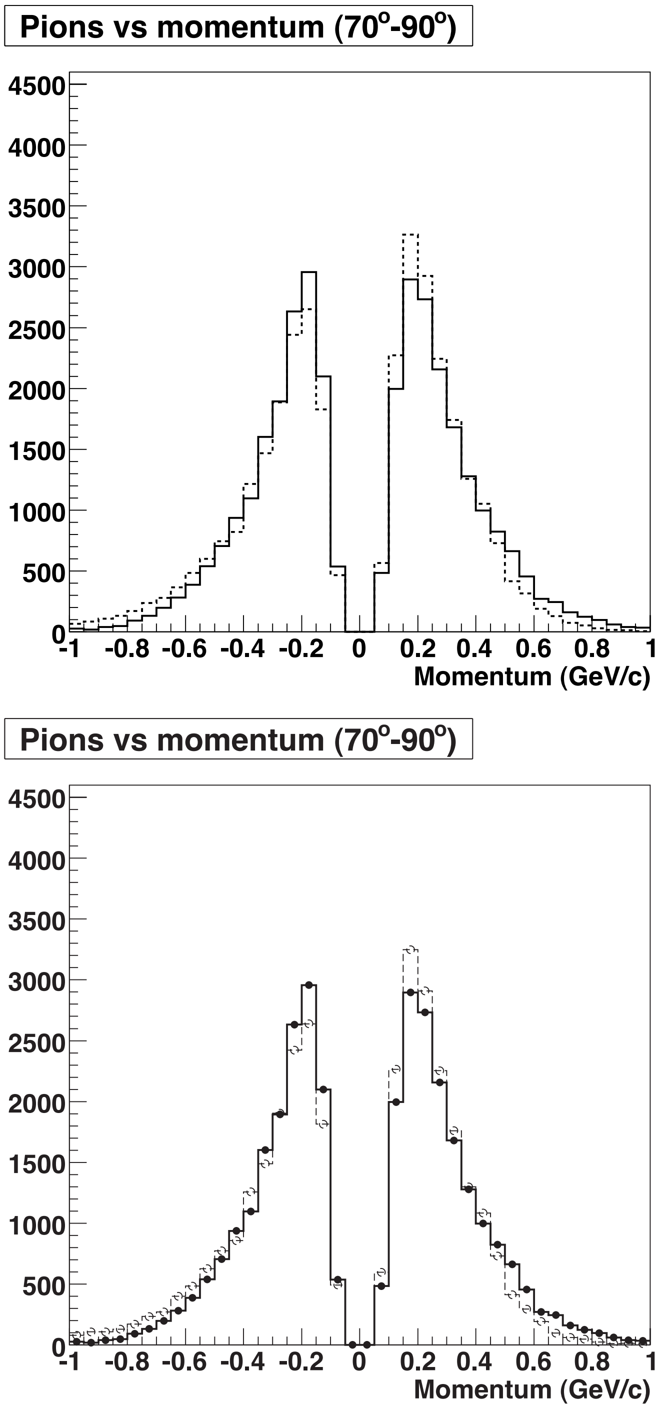

Fig. 12: Number of pions in the polar-angle range $70^\circ < \theta < 90^\circ$ from the CDP analysis (full line) and from the analysis that reproduces the OH analysis (broken line), as a function of the charge-signed pion momentum; the upper panel shows the spectral distortions caused by the OH momentum bias, the lower panel has the effect added from the use of the published OH pion selection efficiency.

6 LACK OF PROFESSIONALISM OR CHUTZPAH?

OH papers burst from claims of ‘high precision’, ‘careful checks’, ‘excellent agreement with Monte Carlo’ and the like. While this may impress the casual reader, a critical inspection reveals the vanity of their claims. For illustration, we give three examples.

6.1 On inconsistent presentation of results

OH present in their papers a plethora of plots, all designed to convince the reader of the high quality of their work. Looking more closely it turns out that the plots do not describe a coherent analysis but often represent inconsistent pieces from different analyses.

A striking example out of many is the claim in Ref. [9] of an RPC timing resolution of 141 ps, a resolution which is obtained from examining the overlap regions between two adjacent RPCs. In the same paper they quote the overall RPC timing resolution as 305 ps[8)], a value that is totally inconsistent with the 141 ps, as is explained in Ref. [21].

6.2 On the ‘500 ps effect’ in hydrogen data

Figure 13 reproduces two OH plots, copies of Figs. 13 and 14 of Ref. [12]. They show for protons Δt, the time of flight measured by the RPCs minus the time of flight inferred from the proton momentum. A negative Δt means either that the proton momentum used for the calculation of time of flight is too low, or the RPC signal arrives too early. It is the latter that OH concluded: *“The difference can only be due to the different response of the RPCs to heavily ionizing compared to minimum ionizing particles. The observed effect accounts for the largest fraction of the absolute values and the shape of the deviations observed in Fig. 13. The remaining difference observed between the points of Fig. 13 and Fig. 14 is of the order of (150 $\pm$ 100) ps at 450 MeV/c, where the error is estimated from the spread of the points for the different pad rings. The central value of 150 ps corresponds to a momentum shift of 4.5% at 450 MeV/c.”*

This quotation from Ref. [12] is of particular interest since everybody can verify that the difference between the Δt’s is not, as stated, (150 $\pm$ 100) ps at 450 MeV/c but rather (350 $\pm$ 40) ps, and thus not insignificant but highly significant (overlayed on OH’s original Figs. 13 and 14 we show in Fig. 13 lines which indicate the 450 MeV/c momentum and the timing differences Δt). Then, the stated momentum shift of 4.5% at 450 GeV/c becomes 10.5%, which amounts to 23% at 1 GeV/c. So OH themselves demonstrate a p_{T} bias which is rather close to our claim that their momentum is biased by 30% at 1 GeV/c. They purport their Figs. 13 and 14 in Ref. [12] to show the absence of a TPC momentum bias—but one reads off the contrary.

6.3 On the ’reasonable’ agreement with E910

OH claim in Ref [16] that their cross-sections are *‘in reasonable agreement’* with those from experiment E910. They write: *“As the Sanford–Wang parametrization does not fit perfectly*

[8)] The 305 ps is determined from the difference of the RPC time measurement and the time that is calculated from the flight path and the momentum reconstructed in the TPC; there is no way to obtain such a bad timing resolution with good momentum resolution.

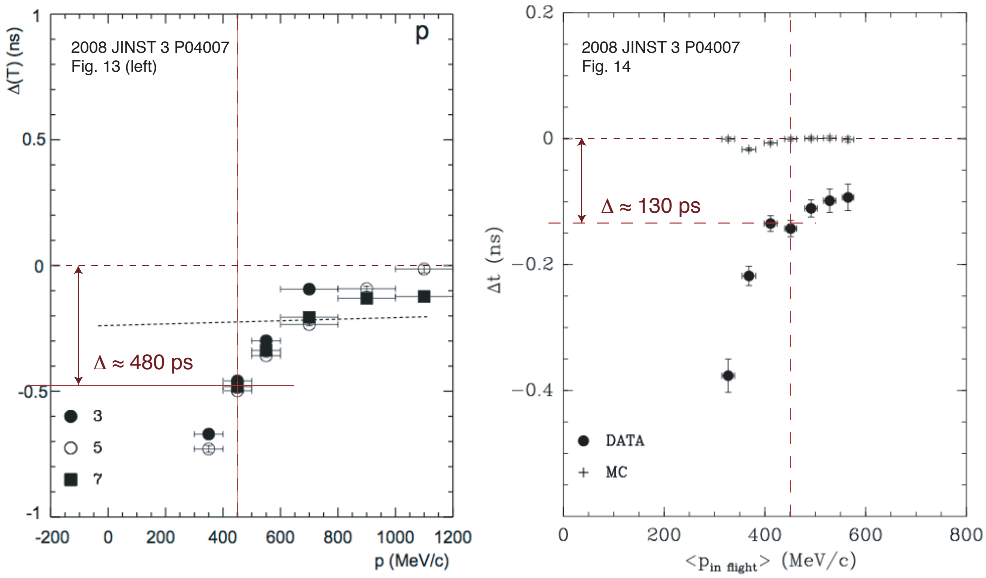

Fig. 13: OH's claim of the compatibility of the proton time advance in the RPCs between protons with their momentum reconstructed in the TPC, and protons with their momentum known from elastic scattering kinematics; the figure is a copy of Figs. 13 and 14 in Ref. [12], with lines and explanatory text added by us that detail the timing differences Δt discussed in the text.

our data, a $\pm 15\%$ *band which contains fully our experimental data points has been chosen for comparison...".* The quoted reasonable agreement is defined by the width of the band they chose. The choice was made to fit the stated purpose. So, by definition it provides reasonable agreement. If OH plotted the published data points rather than their band, a serious discrepancy would be evident.

7 On the 'track comparison' issue

After the SPSC meeting held on 15–16 July 2008, we were informed by the SPSC's HARP referees of their observations from a new comparison of tracks that had been submitted in the course of the SPSC Review by OH and by us. The referees provided us with four transparencies that they had shown and discussed in the closed SPSC session.

We have studied the transparencies and summarize in this memo our assessment of their findings. In short: (i) the reported non-matching of the tracks at the 20% level is expected and anyway not important for the large discrepancies in the π^+/π^- ratios of OH and CDP; (ii) the reported lack of protons misidentified as pions is not conclusive.

The referees expressed concern about

– a non-matching of tracks at the 20% level, and
– missing evidence that tracks identified by OH as 'pions' are identified by us as 'protons'.

On this, we observe the following.

7.1 On 'Non-matching of tracks'

1. Starting from OH tracks and searching for nearby tracks from us: first, it seems that our asymmetric cut of 2° and 10° around the TPC spokes was not applied, this already explains an estimated 8/56 = 14% of unmatched tracks. Second, we point to the difference in the definition of 'good' TPC clusters especially around dead regions, in conjunction with the requirement of a minimum number of 'good' TPC clusters per track. Third, OH's ϕ angle measurement must have a much worse resolution than our ϕ angle measurement because of their improper handling of TPC track distortions. Applying to OH tracks a fixed fiducial cut in ϕ around the TPC spokes means that further tracks from CDP will be unmatched.
2. The referees claim that unmatched tracks are distributed uniformly in ϕ. This is not true, these cases are rather concentrated around the TPC spokes and around dead regions in the TPC readout plane (which vary with time). This means that an important source of non-matching is the requirement of a minium number of 'good' clusters. Near the TPC spokes this number is already near threshold, and even 1–2 clusters which we reconstruct differently or reject by pattern recognition are crucial for the decision to accept the track.
3. The referees observe that *vice versa* 20% of OH tracks are also not found when starting from CDP tracks. We suggest to consider the following. OH provided only tracks identified as pions, with $p_{\mathrm{T}} < 0.5$ GeV/*c* and $p < 0.8$ GeV/*c*, but we provided all tracks irrespective of their identification probabilites. Therefore, the OH cuts of $p_{\mathrm{T}} < 0.5$ GeV/*c* and $p < 0.8$ GeV/*c* must have been applied to our tracks. As a consequence, many of our tracks will not be matched because of OH's much worse p_{T} resolution.
4. We wish to recall that the OH and CDP analysis algorithms are totally different: suppression of cross-talk noise, cluster reconstruction, selection of good clusters, pattern recognition, precision of cluster position, distortion corrections, track fit, and track selection cuts (including minimum number of TPC clusters).
5. The non-matching of tracks is not expected to depend strongly on track charge, and indeed the referees showed themselves that this expectation is borne out.

Altogether, we consider the non-matching of tracks at the 20% level in no way surprising and anyway not an important issue for the understanding of OH's wrong π^+/π^- ratio.

7.2 On 'No protons misidentified as pions'

1. In order to determine the number of tracks of a given species, for example pions, all tracks weighted with their probability must be added up in our analysis. A cut on the probability of belonging to a given species is not consistent with the rationale of our particle identification. Also, we recall that there are not only pions and protons but also electrons.
2. OH identify tracks by $\mathrm{d}E/\mathrm{d}x$ only, time of flight is ignored in their analysis. Particles below a certain curve in the $\mathrm{d}E/\mathrm{d}x$ versus momentum plot are by definition pions. Therefore, for positive tracks identified by OH as pions and matched by a CDP track, it is expected that we identify some of OH's 'pions' as positrons. Since the momentum of positive tracks is biased toward smaller value, it is expected that we identify some more of OH's 'pions' as protons.

   As for the misidentification of protons as pions in the OH analysis, one should not examine the pion probability but rather the proton probability since 'appearance' is more sensitive than 'disappearance'. Furthermore, we suggest to examine specifically the regions of small polar angle θ and of large momentum, since the proton population is largest there (examining merely the pion probability and throwing all polar angles and all momenta together will blur the picture).

Specifically, we suggest to study the equivalent of a 3-D plot of pion probability versus proton probability versus electron probability, separately for positive and negative particle charges, for small polar angle θ and for large momentum. Each OH 'pion' would enter this 3-D plot three times with the weight of the respective CDP identification probability.

3. The referees' analysis suggests that the striking differences between OH and CDP in the π^+ and π^- cross-sections are considerably larger than the differences seen in raw tracks. We conjecture that this is caused by OH's tight cut on the π^+ identification which emphasizes purity over efficiency. We quote: *'The cuts were optimized to maximize the purity of the pion sample, accepting a lower efficiency in the selection'*. The quantitative consequences of this tight pion selection cut have been discussed in Section 5.

Altogether, we consider the purported lack of protons misidentified as pions as non-conclusive.

8 EPILOGUE

We sense some pressure on us from the SPSC to 'collaborate' with OH toward common publications. We also sense a reluctance from the SPSC to accept the HARP split as it may be viewed as a precedent for the LHC era.

First, we invite the SPSC to consider the following general points.

From the scientific point of view, the quality of scientific work ranks higher than the label 'official'. The latter is not a guarantee for correct scientific work. A breakdown of the system is possible as the HARP case amply demonstrates. There ought to be an emergency exit.

The SPSC can and should provide guidance in this. More, this is THE occasion for the SPSC to rise to full height, break new ground, and set a landmark. The HARP experience should be analysed with a view to preparing better for the future.

The SPSC face an unusual but noble challenge. They ought to go way beyond stating the trivial, namely that CDP and OH disagree. We expect them to probe deeper, to understand and weigh the evidence, and to come up with language that will serve as guidance for the scientific community and will be remembered.

Then, we invite the SPSC to consider the following HARP-specific points.

HARP cross-sections are not earthshaking but of considerable practical importance.

It was OH's decision and not ours to proceed with the publication of their wrong results. The *fait accompli* of the publication of their wrong calibrations and cross-sections pre-empted any discussion.

OH's intentional and complete disregard of our scientific work and their unacceptable attacks on our scientific property and personal integrity have left their marks and removed any prospect of reconciliation. The HARP split into two independent entities is a reality and maintaining illusions is counterproductive.

Nobody, not we, not CERN Management, not the Review Board for HARP (Foà Committee), and also not the SPSC, have been able to change OH's attitude.

The OH and CDP analyses developed over four years independently of each other. Any concept of a 'merger' or a 'common analysis' is wishful thinking and unrealistic. There is not one piece of OH analysis software that we would be interested in for our analysis.

The commitments of HARP–CDP group members arising from the start-up of the LHC and other circumstances mean we have no more time to lose. We consider that we have already invested enough time into the understanding of OH's wrong analysis procedures.

In the interest of the scientific community who will wonder which results they can rely upon, we insist on maintaining the dividing line between results from the 'HARP Collaboration' and results from the 'HARP-CDP group' as clear-cut and as visible as possible.

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